theory is certain to form the basis of a careful and complete series of investigations, not only in Europe, but also in those regions in Africa and America where pellagra also occurs, these experiments and results on the destruction of the Simulium larvæ will be of no little practical importance in the prophylaxis of the disease, whether a definite pathogenic organism is discovered, or the case proves to be analogous to that of Stegomyia or yellow fever. C. GORDON HEWITT.
Division of Entomology, Ottawa, November 21.

The Song of the Siamang Gibbon.

THE Zoological Society has recently received on loan an almost adult example of the siamang (*Symphalangus syndactylus*); and since I can find no adequate description of the voice of this ape in the books I have consulted, I think the following description may be interesting to readers of NATURE.

The siamang differs from all other gibbons in having a pair of laryngeal vocal sacs visible externally on the throat as an undivided pouch of loose skin. When the animal is in full song the pouch becomes inflated into an immense oblate spheroid much wider from side to side than from above downwards, and comparable in size to the entire head of the gibbon. A feeble imitation can be made of the booming that comes from this pouch by uttering a guttural monosyllabic "*ooo*" with cheeks inflated and lips compressed. It is not unlike the sound produced by a large bubble of air bursting on the surface of water confined in a narrow space like a rain-water pipe. In addition to this there are two very distinct cries apparently quite independent of the vocal sac and uttered with the mouth open. One is a shrill, piercing bark, like the monosyllable "*haow*," cut off sharply by the abrupt closing of the lips. The other is a prolonged, unearthly wailing shriek—"*ahh—o*"—resembling more than any familiar sound to which I can compare it the "*miaou*" of a cat multiplied ten times in volume. It starts on a high pitched note with the mouth widely astretch, and gradually descends the scale as the jaws are closed. There are two variations of this shriek, one being a note or two higher and more piercing than the other.

The song usually begins with a low and gentle booming punctuated by an occasional staccato bark. As the excitement rises the ape starts to move, and swings round the cage barking vigorously and repeatedly, and now and again uttering the wailing shriek, the loud booming from the now fully expanded vocal sac going on all the while like a resonant bass accompaniment. The noise is deafening and terrific, and I shall not easily forget the consternation of the chimpanzees and the look of mild surprise that pervaded the usually expressionless faces of the orangutans when they heard it for the first time in the apes' house.

The voices of Mammalia have never, I believe, been carefully studied and compared; yet they are worthy of the closest attention as a criterion of specific relationships. The cry of the siamang, for instance, is quite different from that of the Hainan, Hoolock, and Wau-wau gibbons, and each of these species has its characteristic song. I have elsewhere pointed out that the bray of Grévy's zebra betrays pronounced asinine affinities, and equally forcibly attests remoteness of kinship between that species and the quaggine Equidæ; that the likeness between the roar of the lion and the tiger on one hand and of the jaguar and the leopard on the other confirms the conclusion that these species are respectively closely allied, and that these four great cats form, with the probable inclusion of the ounce, a special group of Felis characterised by a roaring voice correlated with a peculiarly modified hyoidean apparatus; that the friendly purr practised by the puma, cheetah, caracal, common cat, and other species which, be it noted, never roar, distinguishes them from lions, tigers, and leopards, which never purr. To the casual observer the Cape hunting dog (Lycaon) is more like a hyæna than a wolf, but the moment he barks and growls it is needless to look at his teeth and skull to detect his cousinship to Canis; and I have recently noticed identity in all essential respects between the raucous growl of a frightened cervine wallaroo (Macropus) and that of a nervous Tasmanian wolf (Thylacinus). In this last instance we have vocal likeness associated with deep-seated ordinal resemblances, and apparently persisting despite great divergences in other structural features and in habits.
Zoological Society. R. I. POCOCK.

On the Simultaneity of "Abruptly-beginning" Magnetic Storms.

IN the first number of *Terrestrial Magnetism and Atmospheric Electricity* for the present year, Dr. Bauer has written two papers, in which he believes he can prove the following (p. 20) :—

"Magnetic storms do not begin at precisely the same instant all over the earth. The abruptly beginning ones, in which the effects are in general small, are propagated over the earth more often eastwardly, though also at times westwardly, at a speed of about 7000 miles per minute, so that a complete circuit of the earth would be made in $3\frac{1}{2}$ or 4 minutes."

Dr. Bauer bases this result upon an investigation of two magnetic storms of Birkeland's "positive equatorial" type, namely, the storms of May 8, 1902, and January 26, 1903. In the latter he makes use of a table in Birkeland's "The Norwegian Aurora Polaris Expedition, 1902–3."

In the following number Dr. Faris made a more thorough investigation of this circumstance, taking fifteen different abruptly beginning storms, recorded at the Coast and Geodetic Survey magnetic observatories, in which he considers that he found Dr. Bauer's result confirmed.

Upon this foundation Bauer then develops the "Ionic Theory of Magnetic Disturbances" (*loc. cit.*, p. 111), of which the principal advantage over Birkeland's corpuscular theory is supposed to consist in the being able to give a natural explanation to time differences such as these, which Birkeland's theory, in his opinion, cannot do.

Notices of these papers appeared in NATURE of August 11.

As it appears that a number of the perturbations described by Dr. Faris are some that I studied last summer when making an investigation of magnetic equatorial storms at the magnetic observatory in Potsdam, a comparison may be of some interest. I determined also the time of the commencement of a number of positive equatorial storms as accurately as possible for another purpose, and without any knowledge of Dr. Faris's work, so that the measuring of the time was entirely independent of it, a circumstance which may be worthy of note.

It may be remarked with regard to the exactness with which the time can be determined by the Potsdam curves that the length of an hour upon the magnetograms is about 20 mm., and that thus one minute answers to about $\frac{1}{3}$ mm. If we then take into consideration all the errors that may creep in because the curves, the time-marks, and the points considered are not so sharply defined as might be wished, and further all the errors that may be due to changes in the paper in developing, owing to the fact that the paper has perhaps not laid quite straight on the roller, &c., it will be evident that where there are no exact automatic time-marks upon the curve itself, one minute will at any rate be the *lowest* limit for the accuracy that under favourable conditions can be counted upon.

There might very easily be an uncertainty of several minutes if, for instance, the base-line is not exactly straight, but is slightly curved, if the parallax cannot be determined exactly, and so forth. Unfortunately, neither Dr. Bauer nor Dr. Faris has stated anything as to how the time in the various cases can be given exactly, a point upon which, it would be thought, it was highly important to be clear.

In the equatorial storms that I have studied, and especially those that are also found in Dr. Faris's Table I. (*loc. cit.*, p. 101), the point at which they commenced is especially clear in H. The deflections in D and Z, on the other hand, are very slight, and in consequence the beginning there is far less clearly defined.

It is therefore the beginning in H that is especially suitable for employment in a comparison such as this, and this was what I especially investigated. It will *a priori* be perceived that the results obtained by employing the other two components must be far more uncertain. In the table below I have compared the means of the values found by Faris for the five American stations that he has considered with those I measured out by the aid of the

Potsdam curves. Finally, I have also given the difference (d_{F}) between the greatest and the smallest time given in Faris's Table I. for the commencement in H at the American stations (Greenwich mean time is employed).

Date		America h. m.		Potsdam h. m.		Diff. m.		d_{F} m.
1906, July	29	19 56·12	...	19 57	...	− 0·88	...	3·7
1907, ,,	10	14 22·92	...	14 22·5	...	+ 0·42	...	3·8
,, Oct.	13	7 42·36	...	7 42·5	...	− 0·14	...	3·9
1908, Sept.	11	7 20·82	...	7 20·3	...	+ 0·52	...	1·3
,, ,,	28	8 42·00	...	8 42	...	0·00	...	2·3
,, ,,	29	1 31·68	...	1 31·8	...	− 0·12	...	3·4
Mean						− 0·03		
Mean of numerical values						0·35	...	3·07

It will be observed that all the differences are considerably below the error-limit which, according to the above, must be reckoned upon, and the difference is as frequently one way as the other.

These figures seem to me to show clearly that in these cases the magnetic impulse occurs, at any rate, very nearly *simultaneously*; in any case there cannot be time-differences of such a magnitude as in Dr. Faris's opinion there are—for July 10, 1907, he even assumes that the storm would take 11·6 minutes to encircle the earth. Further, we see that the *greatest* difference between Potsdam and the mean of the American stations, 0·88m., is only about two-thirds of the *smallest* difference, d_{F}, between the times at the American stations, 1·3m. This circumstance, and the fact that the relation between the numerical means of these time-differences is as 0·35 : 3·07, would seem distinctly enough to show that the great time-differences observed by Dr. Faris can only be due to inaccuracy in the determination of the time, and that the error-limit must be considerable.

Further, if we consider the foundation that Dr. Bauer has employed for the determination of the rate of propagation in the case of the storm of January 26, 1903, it must, I think, be deemed as weak and uncertain as the above-mentioned, which I was able to control. Birkeland, in speaking of the table employed (*loc. cit.*, p. 63), says :—

"The table shows that the time varies so little with the geographical position that it would be premature to draw conclusions from it. The slight differences may be ascribed to inaccuracies in the determinations of time on the magnetograms; for we see that if a difference in time for a certain point appears between two places, this difference is maintained for all the points, a circumstance which seems best to be explained by an inaccuracy in the statement of the time. We may conclude from this that the serrations appear simultaneously, or rather, the differences in time are less than the amount that can be detected by these registerings. . . . The above question, which is of great importance, cannot be definitely decided until we are in possession of rapid registerings."

Bauer holds, however, that by taking groups of means he can demonstrate, clearly and surely, time-differences that would prove that the cause of the perturbation was transmitted eastwards at a rate of 6400 miles per minute.

I also last summer determined the commencement in H of this perturbation in Potsdam, and found the time to be 8h. 53m. Greenwich mean time. I moreover had the opportunity of going through the curves upon which Birkeland's table was based. From these it appeared that the times for the comparative correctness of which there was some guarantee were from the five following places :— Toronto, Kaafjord, Potsdam, Dehra Dun, and Bombay. As regards the other stations, it may be remarked that from Honolulu, Baldwin, and Cheltenham there were only received Indian-ink copies without hourly or two-hourly automatic time-marks. The parallax there could not be determined accurately, and the uncertainty in the time-determination must be considered to be relatively very great.

In the copy of the curve for San Fernando the base-line was a little curved. In that for Batavia the curve and the base-line were very faint; the parallax could not be determined with sufficient precision, and the time-marks were also rather indistinct. A new determination of the

time of beginning which I have just made gives as the result 8h. 52·4m. for San Fernando and 8h. 52·8m. for Batavia. In the table these times are given as 8h. 54·3m. and 8h. 54·9m. respectively, a fact that demonstrates the uncertainty which attaches to these hours. At Christchurch it seems from the D and Z magnetograms as if the clock on that day was about 1·5 minutes too fast, so that the value 8h. 54·8m. given in the table probably should be reduced to about 8h. 53·3m. Further, the beginning of the base-line and the time-marks for the H curve were rather unsharp.

In addition, it may be remarked that the thickness of the curve at Bombay was considerable, about 0·9 mm., thus causing the commencement of the storm to be somewhat less clear; but, on the other hand, there were two-hourly automatic time-marks upon the curve itself, a circumstance which is of great importance in exact determinations of time.

If we now omit those that we already know to be very uncertain, we find the following times of beginning, putting Dehra Dun and Bombay together :—

Toronto		Kaafjord		Potsdam		Dehra Dun and Bombay		Diff.
8 52·6	...	52·6	...	53	...	53·3	...	0·7

Thus the greatest difference is considerably lower than the error-limit, and this would be still less if, as would indeed be best, we attach more weight to Dehra Dun, where the curve is exceedingly clear, than to Bombay. If we attach double the importance to the former, we find 53·1m. instead of 53·3m., and the difference will then be reduced to 0·5m.

It seems to me, also, that this last method, where the conditions are as they are here, must give a far more certain result than that which Bauer has employed.

The remaining characteristic points on the curve seem to me to be too indistinctly defined to allow of being employed in cases where the differences are as small as they are here.

Of the storm of May 8, 1902, I have no special observations that could serve to control Bauer's result. As regards Potsdam, however, I have a determination of its beginning in H, which I also made last summer before reading Bauer's paper. I found the time to be 11h. 58m. Greenwich mean time. Bauer, however, in his table gives it as 12h. 0m. It seems to me that this difference of two minutes is characteristic of the uncertainty that attaches to these statements. When Bauer finds that the weighted mean of all European stations is 11h. 58·24m., it looks as if my determination were the best. When such great differences can be found in the measurement of the same curve, and the Potsdam curves must, I suppose, be considered to be among the most trustworthy of all, how great must be the uncertainty that attaches to the others?

There seems from this, at any rate, to be by no means sufficient data to justify the conclusion that the magnetic storms are generally propagated round the earth in from about 3½ to 4 minutes, and the theory that Bauer mainly bases upon this we must be allowed to regard with corresponding scepticism.

But even if there are no such great time-displacements in these "abruptly beginning storms" as Bauer thinks, there is, of course, a possibility that small time-displacements might exist. This question, which is of such great importance for a full comprehension of the nature of the magnetic storms, can only, however, in my opinion, be solved, as Birkeland has suggested, by rapid registerings. It would be comparatively easy, moreover, to carry some such arrangement into effect by means of a number of stations—at least three—where a short or long period was registered continuously with *very sensitive apparatus* and with frequent and exact *automatic time-marks upon the curve itself*. This was the more easy of accomplishment from the fact that, for the solution of the present question, it was only necessary to register H in this manner. It would then be possible to obtain a sure foundation for reflections of the kind that Bauer makes in his last paper, reflections that, however interesting they may be, must, from what I can understand, be said to be in no small degree premature.   O. KROGNESS.

Universitetets fysiske Institut, Kristiania.